in the present paper as "common warning colours, and the author proposes for them the term *synaposematic*.

Not the least satisfactory feature of the present summing up of the position by Prof. Poulton is the distinct convergence of the evidence in favour of the natural selection theory which has been accumulated since 1879. The sacrifice of a certain percentage of individuals to the inexperience of their enemies was an assumption on Müller's part, and the present writer well remembers pointing out in a letter to that eminent naturalist that his case would be enormously strengthened if he would make observations on the spot. The result was a long series of a distasteful *Acraea*, collected by Müller in order to show that bird-pecked wings were of frequent occurrence. Much evidence of the same kind has been since obtained, and a most valuable series of experiments conducted by Mr. Finn, in India, during the years 1895-96-97, and published in the *Journal* of the Asiatic Society of Bengal, have led that author to the conclusion that unpalatable forms are by no means altogether free from attack.

It must be further borne in mind that in 1879 the question of the non-transmission of acquired characters had not been brought into prominence. It was tacitly assumed in the theory of Bates that a knowledge of edible and inedible types could be transmitted by heredity. It is remarkable that Müller, by virtue of his hypothesis, should have unconsciously challenged this tacit assumption by suggesting that young birds had to learn by experience, and did not derive their knowledge of eatable and distasteful forms by heredity. The whole tendency of Prof. Lloyd Morgan's work of late years has been to confirm this suggestion by actual observation and experiment; and Mr. Finn, also, in summing up his results, states that "each bird has to separately acquire its experience, and well remembers what it has learned." Thus the Müllerian theory of 1879 has now been placed on a psychological basis of well-ascertained facts.

Those who still believe that common warning colours can be explained by internal or external causes, as defined in the present paper, will, we imagine, find the ground crumbling away from beneath their feet if they will seriously weigh the arguments set forth by Prof. Poulton. What series of external causes in nature are there, for example, which can so act upon an organism as to modify only those superficial characters which are required to bring about a resemblance to another form while leaving all other characters unmodified? To attribute such modification to independent evolution by virtue of innate tendencies or laws of growth or internal forces, appears to the writer to be substituting mysticism for scientific explanation. What external agencies can be conceived which shall, while acting without visible result upon the early stages of all kinds of insects, culminate only in a resemblance between the imagos? The external conditions of life are imposing themselves during the whole of the larval and pupal existence, and yet these forms remain quite distinct, while the imagos come forth at once with all their disguising characters perfected.

On considering again the undoubted fact that in many cases of mimicry and common warning colours the female only is affected, the inadequacy of any explanation depending on direct action of environment or internal evolutional "tendencies" becomes strikingly apparent. So also, as Prof. Poulton illustrates by a most remarkable set of examples, when insects of different orders resemble each other, the superficial similarity must necessarily be brought about by the most diverse kinds of modification of parts. To attribute such distinct and diverse modifications of form, directed towards a common end, to similarity of external forces or internal tendencies, seems to the writer to be a straining of hypothesis beyond any degree of rashness attributed to the supporters of natural selection. What natural agency can be imagined that will account for the production of a similar colour in two or more species—in one form by developing pigment, and in another by developing striation of surface, so as to produce the same chromatic effect, excepting selection which works only for advantageous results irrespective of means? Even within the same order, where the resemblances might be more reasonably supposed to be due to similarity of external conditions, the likeness is superficial only, and is brought about by the most diverse means. There is apparently no chemical relationship between pigments which produce the same visual effect in mimetic butterflies of different families. A visual resemblance is required only by natural selection; external and internal causes have been incompetent in such cases to modify the more deeply concerned physiological processes so as to produce similarity of appearance by identity of pigment. Such a character as transparency of wing, also, is shown to have been attained by several distinct methods; by reduction in the number of scales, by reduction in their size, by loss of pigment, by being set up on edge instead of lying flat, and so forth. Any common set of forces, external or internal, which can bring about the same result, viz. wing transparency, by such diverse methods is simply inconceivable.

We have given only a few illustrations of the arguments which the author makes use of in this paper to dispose of the theories which have been advanced by way of substitutes for natural selection. As Prof. Poulton says in conclusion: "The review of the whole subject during the past thirty-six years increases our confidence in the theories of Bates and Fritz Müller, while it disposes of all alternative hypotheses."

It should be added that many new examples of mimicry and common warning colours—some of them of the most striking character—are given in the paper. More particularly will English entomologists be interested in the resemblance of the young larvæ of *Stauropus fagi* to an ant, and of the similarity in appearance and habit of the young larvæ of *Endromis versicolor* to saw-fly larvæ.

R. MELDOLA.

PROFESSOR CHARLES FRIEDEL.

FRANCE has lost one of her most distinguished chemists in the person of Prof. Charles Friedel, member of the Institute, who died at Montauban on April 20. He was born in Strassburg on March 12, 1832. His father was a banker; his mother was the daughter of Dr. Duvernoy, well known in his day as a scientific man. He distinguished himself so greatly in his studies that he took his degree of Bachelor of Science with special honours. Desiring to follow science as his profession he went to Paris, and gained the special esteem of M. de Sénarmont, who caused him to be appointed conservator of the mineralogical collections at the École des Mines. He worked in the laboratory of the distinguished chemist M. Adolph Wurtz, also a native of Alsace, at the École de Médecine. In 1856 he married Miss Kœchlin, by whom he had five children, one of whom, George Friedel, is known as a professor at the mining school of St. Etienne. Mrs. Friedel died in 1871, at Vernex, where she had retired during the Franco-German war; and her husband, who was shut up in Paris, knew nothing of the sad event until after the city capitulated. He was married again, in 1873, to Mlle. Louise Combes, whose father was a member of the Institute of France, and who, with their son and a large circle of relations, now mourn his recent decease. To return to his professional distinctions: in 1869 he became Doctor of Science; two years after he received a high appointment at the École Normale Supérieure. In 1876 he became Professor of Mineralogy at the Faculté des Sciences, at the Sorbonne; and in 1878 he received the

distinguished honour of membership of the Institute (Academie des Sciences). In 1884 he took the position of his late master, Prof. Wurtz, in the chair of Organic Chemistry at the Sorbonne. His merits were fully recognised in this country. In 1876 he became a foreign member of the Chemical Society, and four years later he received the Davy Medal of the Royal Society. In 1894 he made one of his rare visits to England to receive the degree of D.C.L. of Oxford University, an honour which he acknowledged as a great encouragement.

His influence on the advance of science was of a two-fold character: as a teacher, and as an original investigator. He was not known as a popular lecturer or writer upon science; but he had the happy faculty of infusing the love of science into the minds of the large number of students who attended his professorial lectures or worked in his laboratory. This result was no doubt greatly enhanced by the respect and personal attachment with which he was regarded. The advancement of education was in fact one of the objects of his life. This was evidenced by the successful efforts he made in promoting the École Alsacienne in 1874, which, to use his own words, was "designed to react against the exclusively literary and formal instruction, and directed in a Protestant and Christian spirit, without having any denominational colour." Its aim was to develop in each scholar the faculties which belong to him, and to arouse a spirit of observation and scientific curiosity. Natural science has, of course, an honoured place in the curriculum. He watched over this school with great interest, and helped to make it one of the best in the capital of France. The technical side of science also engaged his attention; and he had a large share in founding at Paris, three years ago, a laboratory of practical chemistry applied to industry, at the Sorbonne, and to which he gave special attention. He was one of the founders of the French Chemical Society. It is said also that the French Association for the Advancement of Science owes its origin to his suggestion; at any rate he came to the meeting of the British Association at Brighton in 1872 to learn the details of its working, for the benefit of the French Association which was to be inaugurated at Bordeaux in September of that year. The two Associations, though very different in their constitution, are carried on in much the same manner. M. Friedel generally took an important part in the French Association's annual meetings. In the last of the numerous letters that I received from him, he made reference in hopeful terms to the approaching meetings of the two Associations at Dover and Boulogne in September next, and to the efforts which were in contemplation to bring together the savants of the two nations.

Throughout the whole of his career he carried on original research, the results of which are published in about one hundred papers communicated to the Academy of Sciences and other learned societies. Some of these refer to the artificial formation of felspar and albite, crystallised quartz and other minerals, and to the dimorphism of zinc blende; but by far his most important work has reference to the carbon compounds, and the long controversies which raged over the question of their constitution, and how it should be expressed. His first paper seems to have been a contribution, in 1857, bearing on the constitution of acetone. This was followed by others on lactic acid, glycerine, propylene and other members of the three-carbon family. The relation of these bodies one to another, and to their isomers, led to much fruitful controversy. To him, in fact, is due in great measure the introduction of the new views of atomic valency, of which the chief apostles were Cannizzaro and Kekulé. In France these ideas were not readily received; the chief advocacy of them came from the laboratory of Wurtz, and although Friedel had not the enthusiasm and brilliancy of the master, his expositions and arguments were wonderfully clear, and his experiments in support of them very convincing. Among these was the production, in conjunction with Ladenburg and Crafts, of a number of compounds of silicon and titanium showing the quadrivalence of these elements and their chemical analogy with carbon. In this way they broke down the barriers between organic and inorganic chemistry, and showed the generality of the laws of chemical combination. During these researches he was fortunate in discovering a new method, by means of chloride of aluminium, of bringing about the synthesis of organic compounds, often producing hydrocarbons of a highly complex character.

With the rapid advance of chemical knowledge, especially in the organic department, and the gradual growth of chemical theory, the nomenclature was found to be inexact and often misleading. Hence in 1892 a congress of chemists was held in Geneva to revise the nomenclature. Leading representatives of chemical science from many countries met together, and Friedel was appointed president. The recommendations arrived at were published in Wurtz's Dictionary of pure and applied chemistry, which was carried on under the direction of Friedel.

But he did not confine his work to scientific teaching and investigation. Born in a Protestant family, he seems from his youth to have adopted the religious principles in which he was brought up. He sympathised with all Christian, philanthropic or patriotic movements of his country, and took an active part in many of them, especially those that related to the welfare of young men. Those of us who knew him intimately will feel disposed, like the President of the Academy in announcing his death, to dwell not so much on his great scientific achievements as on the amiability and uprightness of his character and on the moral worth of his personality.

J. H. GLADSTONE.

CHARLES NAUDIN.

CHARLES NAUDIN, whose contributions to science extend over the last sixty years, died on March 19, at Antibes, at the age of eighty-four. A systematist by his studies of the orders *Melastomaceae* and *Cucurbitaceae*, a biologist by his work on hybrids, he is perhaps best known by many contributions to economic botany.

The bravery with which he met the hardships of his life wins admiration. His father, a schoolmaster, ruined himself; his mother died when he was but eight years old. At Montpellier, while working for a degree, he served as usher in small establishments: the degree gained, he became a teacher at Château-Chinon, then at Cette. In 1839 we find him at Paris earning his living by teaching, by copying commercial letters, and lastly as a gardener at the Jardin des Plantes, burning the midnight oil in order to obtain his licentiate in 1841 and the degree of Doctor of Science in 1842.

After helping Saint-Hilaire with his flora of South Brazil, Naudin became professor of zoology at the Collège Chaptal. But, when success seemed assured, severe facial neuralgia and an incurable deafness, worse than the neuralgia, cut him off from free communion with his fellow-men. Forced back from his course, he applied himself again to herbarium-work, and the study of the *Melastomaceae*—an order richly represented in Brazil—gave him employment till 1849.

Five years later Decaisne made him his aide-naturaliste, and under his stimulus Naudin commenced the experiments on hybrids which secured his reputation. Darwinism had disturbed science; and Decaisne, who, like others, was asking what are species, had commenced to experiment on variability with admirable patience by growing pears from seed. Naudin chose the Gourd